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Psychotherapy and the Experimental Approach

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MY APPROACH TO psychotherapy has always been directed by certain axiomatic beliefs which I formed quite early on in my career, when I was asked to create an academic Department which would lay the foundations for a proper training in clinical psychology in the United Kingdom. Until then there was no profession of clinical psychology in existence, and no clinical psychologists giving a service for adult patients; there did exist a number of educational psychologists, concerned entirely with school children referred to child guidance clinics, usually for a mixture of educational and personal problems. In order to study the existing practices in the only country which at that time had a flourishing program of clinical psychology training, I spent a year in the U.S.A. as visiting professor at the University of Philadelphia—an appropriate choice, as clinical psychology may be said to have begun there, under the guidance of L. Witmer! I also travelled extensively, and visited many of the better known centers; I think that my experiences then, and during a later visit when I held a visiting professorship at Berkeley, enabled me to form some reasonably accurate impressions of American practices.

Clinical psychology seemed to be based very largely on psychoanalysis, rather than on psychology; it seemed to rely heavily on psychotherapy, aided by a variety of projective techniques; and it seemed to be an ancillary disci-

pline to psychiatry, which imposed on it the peculiar nomenclature and nosology with which we are all familiar. Coming to this scene from experimental psychology, I formed the following conclusions; these conclusions were also based of course on a detailed reading of the literature, and some of my conclusions have appeared in printed form at various times (Eysenck, 1949, 1950).

(1) Clinical psychologists are trained (or should be trained) as psychologists first and foremost; that means that they should impose the strictest controls on their empirical work, and seek for proper verification and proof before accepting any teaching or method for practical use. (2) The evidence for psychiatric systems of diagnosis, nosology, etc. is poor, subjective, and ratings based on these systems are too unreliable to be acceptable. (3) The evidence for psychoanalytic theories is equally poor and largely non-existent. (4) There is no evidence that psychotherapy, whether Freudian or other, cures or improves patients any more quickly or soundly than would happen anyway through spontaneous remission. (5) There is no evidence that projective techniques possess any reasonable reliability or validity. (6) Research in these subjects is technically of a very poor quality, and cannot possibly give us the sort of reliable and worthwhile information on which alone a scientifically acceptable discipline can be built. (7) Clinical psychologists have sold their experimental birth-right for a mess of psychiatric pottage, and can only regain it by becoming proper psychologists, depending for their effectiveness on the findings of experimental psychology.

Natural Science Theory of Neurosis

What does this prescription mean in practical terms, and particularly insofar as psychotherapy is concerned? It meant two things to me. In the first place, we must try to establish a natural science theory of neurosis, i.e. a theory which rests firmly on theoretical knowledge gained in laboratory experiments. And in the second place, we must develop methods of treatment of neurotic disorders which are in turn based on this theory of neurosis, and equally use knowledge gained in laboratory experiments to eliminate the disorders complained of by the patient. Psychotherapy in the classical sense fails on both counts; it has not given us a proper theory of neurosis which is testable by natural science methods of proof, and it has not given us a method of treatment which is demonstrably successful.

It was for these reasons that my colleagues and I developed what we called "behavior therapy" during the years between my first visit to the U.S.A. in 1949-1950, and the publication of my first article on these new methods in 1959 (Eysenck 1959, 1960). Wolpe, Skinner and others independently pursued the same goal, and arrived at some of the same conclusions; on the whole, however, they tended to concentrate on a single method of treatment (desensitization, operant conditioning), whereas we tended to subsume under

the term "behavior therapy" all methods derived from modern learning theory, i.e. including, as well as the above, methods of "flooding," modeling, negative practice, bio-feedback, etc.

Neurotic Behavior and Conditioning

The theory of neurosis which we finally arrived at essentially asserts that neurotic behavior is caused by a process of Pavlovian conditioning, and that the conditioned responses are often preserved by a process of operant conditioning; the details of this theory have been published in my book (with S. Rachman) on *The Causes and Cures of Neurosis* (1965). If this is true, then obviously the cure of neurotic disorders depends on some form of *extinction,* and clearly the experimental literature on learning and conditioning furnishes us with many suggestions as to how best this can be accomplished. Of the success of this venture there can now be little doubt; we have carried out several studies in which we have compared the progress of matched groups of neurotic children, or adults, some of whom were randomly assigned to orthodox psychotherapy, while others were treated by behavior therapy. Provision was made for independent assessment by an experienced psychiatrist (usually trained in psychotherapy and favorable to it), as well as for empirical (behavioral and physiological) assessment of final outcome. The results showed that behavior therapy (a) worked better, in the sense of reducing "symptoms" to a much greater extent, and (b) worked faster, in the sense that much less time was needed to produce the desired effect. A third effect was looked for, but not discovered, namely (3) that in spite of considerable effort, no evidence was found of symptom substitution or relapse.

These experimental studies should be seen as complemented by studies in which control groups were not employed, but where the normal outcome was well known. As an example, we may take obsessive-compulsive disorders; here the prognosis was so poor that psychotherapists tended to refuse to take on such cases, as the outlook was very pessimistic. When extinction techniques were used with such patients (in particular, "flooding" and modeling), cures became the rule rather than the exception, and it was particularly notable that the time element involved was incredibly short; within a matter of weeks patients were cured of a long-standing disorder which had resisted long-continued efforts on the part of psychoanalysts and psychotherapists.

It is this kind of *ad oculos* demonstration which has proved to be powerful in persuading psychiatrists working at the Maudsley and Bethlem Royal Hospitals to take up behavior therapy as a normal part of their armamentarium; the close sequence of treatment and improvement (not delayed, as in psychoanalysis, by 4 or more years) makes the causal connection unusually clear. In general, psychiatrists do not read much and show little interest in controlled research; they do, however, tend to pay attention to clear-cut improvement in their patients, especially when such improvement is quite unexpected in terms of previous experience.

My approach to psychotherapy has not changed in the slightest since my first visit to the U.S.A. I believe that psychotherapy of the usual interpretive kind is simply the premature crystallization of spurious orthodoxy, a verbal exercise without any proof of effectiveness. There is now much more literature on the subject, although the quality of the work done has not improved all that much. S. Rachman (1972) has recently reviewed all the literature in his book on *The Effects of Psychotherapy,* and has come to the same conclusion; there was no evidence for the effectiveness of psychotherapy or psychoanalysis at the time I wrote my 1952 paper, and there still is no such evidence. Many attempted refutations of my thesis have been published, but it is noteworthy that these are all argumentative; usually they attempt to disprove my thesis by misrepresenting what I actually wrote. But they clearly fail to do the one thing which would most effectively prove the value of psychotherapy: cite one single study which indisputably demonstrated that psychotherapy succeeded better than no treatment, or behavior therapy, or any other alternative in curing neurotic patients, randomly assigned to these various treatments, of their ailments.

Three Methods of Treatment

It is now twenty years since my challenge was first published; the fact that it is still unanswered seems to me to say much about the value of psychotherapy as popularly understood. (Behavior therapy is sometimes considered to be a variety of psychotherapy; this opposes "physical treatments" as for instance by drugs or leucotomy to non-physical treatments, all of which are lumped together under the term "psychotherapy." I would prefer to oppose behavior therapy to psychotherapy, as well as to physical treatment, thus arriving at three distinct methods of treatment. But the point is inessential, and no doubt eventually psychotherapy will become completely coterminous with behavior therapy.)

What was said in the last paragraph also represents my evaluation of the present status of psychotherapy. If we use the term as opposing traditional practices to those associated with the term "behavior therapy," then I believe that psychotherapy is dying; in fact, this realization seems to be gradually dawning on many of its practitioners who are beginning to disclaim their previous high hopes, and now say that psychotherapy does not cure patients of their "symptoms," but merely makes it easier for them to live with their "symptoms." (The word "symptoms" is put in quotation marks because according to the behaviorist theory of neurosis outlined above there are no "symptoms" indicative of an underlying "complex"; the "symptoms" are not symptomatic of anything—they *are* the neurosis!) Psychotherapists do not explain why anyone should wish to go on living with his "symptoms" when he can get cured of them in comparatively little time by means of behavior therapy;

they tend to complain that behavior therapy cures the patient of nothing but the symptoms. But of course in our theory there *is* nothing else, and it ill becomes those who cannot *even* cure the symptoms to complain that behavior therapists can *only* cure the symptoms!

If we use the term psychotherapy to include behavior therapy, i.e. as a kind of supraordinate classificatory term, then of course my evaluation of its present status changes completely. Now, for the first time, we have available a method of treatment which actually works, which is derived from psychological principles, and which can only be employed properly by trained psychologists with a firm knowledge of modern learning theory. This means that we have succeeded in establishing our proper identity vis-à-vis psychiatry; this is our speciality, this is our acquired skill, this is our kind of knowledge. No longer do we have to copy slavishly the models and methods of "dynamic" psychiatry, or be second-rate citizens in a medical universe. Here is an area where medical expertise is completely irrelevant, and where psychological knowledge alone counts.

Psychologists have for years tried to gain a sense of identity by becoming experts in all sorts of absurd games like Rorschach interpretation and other projective devices; now they have a chance to become experts at something which really has social value and offers tremendous opportunities for all those benevolent instincts and tendencies which cause so many people to take up psychotherapy. Now we can forget about the imaginary fragments of Freudian fantasy and get down to the real nitty gritty of curing real patients of real "symptoms" which are threatening to ruin their lives. My assessment of the present state of psychotherapy, thus understood, is therefore very optimistic; I believe that once the resistance of the older and more rigid members of the establishment to the necessary change has been overcome (as it has been in England, and will no doubt soon also be in the U.S.A.), there is a strong probability of a new burst of life for psychological methods of treatment, carried out by psychologists trained in the application of their science to the problems thrown up by life.

Development of a New Discipline

Granted that psychotherapy will become coterminous with behavior therapy, and that sufficient numbers of well-trained experimental psychologists will come forward to employ their knowledge and skill in the field of clinical psychology, I believe that we are about to see the development of a new discipline which is likely to be of great social importance. It has always seemed to me that psychiatry is not in any real sense a proper medical specialty. Medical specialties are grouped around either a method of treatment (like surgery), or around a particular set of bodily parameters (like ear, nose and throat departments). Psychiatry contains a bewildering mass of quite unrelated dis-

orders, from epilepsy to neurosis, from schizophrenia to Alzheimer's disease, from porphyria to psychopathy to brain damage. There is no single method of treatment to link these together, nor is there any unity discernible among the various disorders dealt with by psychiatrists.

It seems to me that psychiatry could conveniently be broken in two, with behavioral disorders having a psychological origin (i.e. mainly the neuroses, psychopathy, criminality, etc.) on the one hand, and metabolic disorders, brain dysfunctions, etc., on the other. The former group would more properly form part of *psychology*, i.e. clinical psychology, and would be treated by means of behavior therapy; the main point of this treatment would be behavior modification. The other group, properly medical, would form the basis for a reorganized and more unified psychiatry; the main methods of treatment would of course be drugs, with physical methods probably slowly receding. Psychologists would still be useful in working within psychiatry as so defined; but their function would probably be secondary, concerned more with diagnosis than with treatment. An important exception would perhaps be the treatment (by means of token economies) of iatrogenic illnesses, which are so common in psychotic disorders. Psychiatrists would no doubt be unwilling to hand over the treatment of the first group of patients to psychologists, but the whole logic of modern discoveries points that way, and in due course no doubt society will realize that it is not sensible to allow medical people, trained in physical diseases, to treat psychological disorders of a purely behavioral kind, without ever having been trained in the relevant disciplines, such as learning theory, conditioning, etc.

Mobile Behavior Therapy Clinics

The import of these developments on the sum total of human happiness could be considerable. Even now we would be able to wipe out some 80% of all phobic disorders, obsessive-compulsive illnesses, and many other manifestations of neurosis by manning mobile clinics with well trained behavior therapists and sending them around the cities to treat the millions of people who are known to suffer from these disorders, but who do not normally come for treatment to hospitals or private doctors. It has already been shown that behavior therapy can be administered as a form of group therapy; there are other ways and means of accelerating the process and of treating larger and larger numbers in a shorter time than is required now. It should be remembered that behavior therapy is still in its infancy; the number of practitioners who have carried out sensible research into the development and improvement of the various techniques which go to make up behavior therapy could be ticked off on the fingers of one hand. Once we reach the stage where the considerable financial and other resources which at present support training and research into psychotherapy of the narrow type are shifted to the development of behavior therapy, marked advances in technology are likely to take place in a very short time.

I believe that society needs these developments; the burden of neurotic disablement is much greater than most laymen or doctors realize. I hardly need emphasize this point to experts in the clinical field. It should also be appreciated that many doctor-hours are wasted because the patients are not suffering from any kind of physical disorder, but are merely using the physician in lieu of a proper psychological service geared to their neurotic troubles. In this way the scarcity of good physicians could be relieved, by concentrating their energies on those illnesses properly within the orbit of their expertise.

It would be easy to go on discussing the possible social developments consequent upon the discovery of behavioral methods of successfully treating neurotic disorders; I will instead mention one consequence which to me as an experimental psychologist is of equal interest. Laboratory experiments in learning and conditioning have enabled us to set up a model of neurosis, and to develop theories about the extinction of neurotic habits which have been very successful, considering the short period of time over which these developments have taken place. In return, the large number of neurotic patients in treatment can serve the experimentalist as an extremely interesting and important testing bed for his theories, and in addition these patients may throw up new problems which would be difficult to reconstruct in the laboratory. The high degree of anxiety exhibited by patients would not be possible to produce in laboratory research, and consequently we may learn a great deal from our work with patients which could not in the nature of things be learned in the laboratory.

The Problem of Extinction

To take but one example of the sort of thing I have in mind, consider the problem of extinction. According to all the text-books, the presentation of the CS, not followed by the presentation of the UCS, produces extinction. I have argued that this is the model according to which spontaneous remission works, but note that spontaneous remission does not work in all cases–what has happened to those patients who do not remit? It is possible to bring in here the Miller-Mowrer theory of avoidance learning, and this no doubt plays a part; nevertheless, detailed case histories of numerous neurotics convinced me that there was more here than a simple combination of classical and operant conditioning. It seemed to me that extinction did not always follow upon the presentation of the unreinforced CS, and that in many cases incubation or enhancement, rather than extinction, ensued (Eysenck, 1968). Animal work has since established the truth of this proposition; it seems that the precise consequences of the unreinforced presentation of the CS depend on such parameters of the situation as the length of exposure (short exposure–enhancement; long exposure–extinction).

This information in turn has led to improvements in treatment; the technique of flooding ("implosion") for instance only works when presentation of the feared object is continued for a long period of time, and may exacerbate

the neurotic condition if it is too short. In this way behavior therapy and experimental psychology interact, to the mutual advantage of both. This to me is a very important development; nothing of the kind seems to have happened with orthodox, traditional psychotherapy, which has been almost completely divorced from experimental psychology. To effect a reunion of two disparate disciplines like that must be one of the main aims of behavior therapists. They can learn a great deal from the experimentalists, but they can also contribute to experimental psychology.

Many psychotherapists will find it strange to hear such opinions as those incorporated into this article promulgated in the *Journal of Contemporary Psychotherapy.* There is an almost insuperable divide at the moment between psychotherapists and behavior therapists; to my mind this is not a good thing. If our aims are similar (to cure the patient), and if our methods are different, then we should surely benefit from getting together and thrashing out our differences.

Are psychotherapists really familiar with some of the results achieved by behavior therapists, and can they evaluate these in terms of their own experiences? Do psychotherapists read *Behaviour Research and Therapy,* the leading journal of the behavior therapy movement? Do psychotherapists at least try to use some of these new methods in recalcitrant cases, in order to see what might be done; do they invite behavior therapists to give an exhibition of their techniques? Are they really interested in hearing (and answering) the criticisms which behavior therapists have to make of their practices, and their evidence? If they like to think of themselves as scientists, do they behave like scientists in looking for defects in their own theories, do they welcome criticisms which are knowledgable and well-informed, do they correct defects in experimental designs pointed out by psychometric experts? Are they really interested in providing proper evidence for their claims, using proper controls, proper methods of analysis and proper criteria of recovery? Psychotherapists have tended to shut themselves off from the rest of psychology, and to claim superior wisdom not immediately visible to outsiders. I think that one of the most important contributions of behavior therapy has been to make such an attitude impossible; more and more the outer world, the world of patients, of doctors, of government agencies, will intrude into this dream world with direct questions about cost effectiveness, about percentage cures, about time spent per patient. The outcome problem cannot be disregarded forever, and its forcible bringing to the fore by behavior therapists has fundamentally altered the whole situation.

Perhaps it would be best in both our interests if we could get together and arrive at some sort of modus vivendi; to the outer world, at least, the distinction between different schools within psychology is very blurred indeed, and it would be so much better if we did not give ammunition to all those who are opposed to clinical psychology by presenting them with the picture of a house divided against itself. It is this, more than anything, that prevents us from taking advantage of the recent developments which might establish

psychotherapy (in the widest sense) as an important new social discipline. In trying to build bridges, the first essential is of course honesty; such bridges cannot be built on the shifting sands of expediency and pretense.

The criticisms of psychotherapy (in the narrow sense) which I have made over the years may not present an accurate picture of reality as psychotherapists see it, but they have not replied in the past with facts and figures which would be meaningful to me and my colleagues. Perhaps the time has now come for a dialogue aimed at setting up a unified discipline of psychotherapy of which we can all be proud; there is nothing in modern learning theory which says that cognitive factors must not be allowed a prominent part in the establishment of conditioned responses, and personally I am convinced that a proper theory of neurosis must inevitably come to terms with Pavlov's "second signalling system"–perhaps along the lines of Platonov (1959). But a dialogue demands knowledge of the other party's position, and while all behavior therapists have at least some knowledge of the principles of "dynamic" psychology, psychotherapists have not in the past been very open to the incursions of experimental psychology and behavior therapy. It is my profound hope that this will change, and that the stage will be set for a proper dialogue between psychotherapy and behavior therapy; a dialogue which might be extremely useful for both, and which might lead to a much desired reunification.

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